like darkened areas; but, on the contrary, when we consider the high temperature of the sun, we may conceive them to be formed of matter in a glowing state, so that products of condensation such as these could scarcely, if at all, be perceived on the luminous disc of the sun. On the other hand, the author believes that in the cases of the great and still warm planets, Jupiter and Saturn, we see the sun-illumined aqueous clouds that rise in bright belts at the equator. We believe the author's object is to show that, while the visible effects of condensation appear in the atmospheres of Jupiter and Saturn, it is only on the liquid surface that they are exhibited by the sun, and thus its atmosphere remains transparent.

We have, accordingly, in the equatorial zone and in the higher latitudes distinct regions of preponderating atmospheric cloudiness, and between them, like the zones of the trade winds on the earth, lie areas of relative clearness. All this, if I rightly understand the author, is not apparent to the observer, but its effects are seen on the glowing liquid solar surface, where, beneath the unclouded areas, radiation is more induced than in other places, and the formation of sun-spots is the consequence.

To the foregoing causes of atmospheric disturbance must be added the eruptions of hydrogen that are shown by the spectroscope.

On the whole it is the *stillness* and *clearness* of certain parts of the atmosphere that induce the formation of spots; and, as the final result of his arguments, Prof. Zöllner sums up as follows:—

"The sun-spots are slag-like products of a cooling process caused by the radiation of heat from the glowing liquid surface, and they dissolve again in consequence of disturbances of equilibrium in the atmosphere which are brought on by themselves. If these disturbances are not merely local, but of more general extent, then, at the times of such general atmospheric disturbances, the formation of new spots is but little favoured, because the essential conditions of a considerable lowering of temperature are wanting, namely—*stillness* and *clearness* of the atmosphere. When the atmosphere, after the dissolution of the spots, gradually tranquillises, the process begins anew, and it assumes a *periodic* character, while the conditions of the solar surface are to be regarded as constant in the *mean* of lengthened periods. The local distribution of the spots must, according to this theory, depend on the zones of greatest atmospheric clearness, which, as has been shown, are generally coincident with the zones of the greatest development of the spots."

Such, as they appear to me, are Prof. Zöllner's views of the sun's spots; and if, as is quite possible, I have not everywhere succeeded in comprehending him, I freely admit that any misconstruction I have made may be attributable to my own shortcomings rather than to his. At the same time I cannot but regard his style as considerably difficult and diffuse, and not perfect in the logical concentration which is so necessary for the clear enunciation of a theory. In some points his conclusions seem, undoubtedly, to agree with observations—for instance, as regards the vaporous masses that are formed over the spots, and which appear sufficiently attested by their strong absorption lines in the spectrum. In the main, however, I cannot say, so far as I may venture to give an opinion, that he has been more successful than other theorists on the same subject; and among several objections which have occurred to me as affecting his views, I will venture to state the following:—

1. Regarding the establishment of currents in the liquid envelope, Prof. Zöllner affirms that in this way alone could the more heated and specifically lighter portions of the liquid at the bottom make their way to the surface; but this appears to me incorrect when we consider that in freezing water there is an interchange of the upper and lower strata until congelation begins, and this without the intervention of currents.

2. He defines the spots as scoriaceous products floating on the liquid surface. The liquid, however, is moving in a current from the equator poleward, and, if so, I would ask how is it that the spots show no tendency to be carried along with it in that direction? I do not believe that any such general tendency has been observed.

3. He makes no attempt to account for the very striking and suggestive appearances of the penumbra, which led Wilson to regard the spots as openings or depressions in the photosphere. Neither does he try to explain the distinct boundaries of the nucleus, the umbra, the penumbra, the light bridges, nor the deeper shading of the penumbra round its exterior limits.

4. The current cools in its advance poleward, and the polar regions are, as the professor tells us, the coolest parts of the sun. Then if, according to his theory, the spots are the products of cold, why do they not increase in development up to the poles? He assumes, indeed, that the cold induced in the polar regions produces clouds in the atmosphere, which are unfavourable to the production of spots; but they are so only as they check radiation and contribute to heat, and if, notwithstanding this, the polar regions are still found to be the coldest, and if cold is the cause of the spots, there seems a defect in the hypothesis. The professor points to an analogy between the spot zones and the clear zones of the trade winds of the earth. The cases are, however, very different; for our own atmosphere is subject to external influences, namely, the action of the sun, as well as those belonging to the earth itself.

Of course, anything from the eminent pen of Prof. Zöllner must be received with the utmost respect, but I conceive that his theory of the sun-spots, as I have attempted partly to show, presents many difficulties; and I cannot avoid stating my humble belief that, notwithstanding all that has been thought and written on the subject, and in spite of the modern discoveries of some of the constituent elements of the sun, we are but little nearer a true conception of its organisation or economy than the theorists of the days of Hipparchus.

J. BIRMINGHAM

PROFESSOR TYNDALL ON THEORIES OF DISEASES

WE reprint this article from the *British Medical Journal*, since it shows how closely connected are the most abstract inquiries with the most practical questions:—

"The surest basis for Medicine is upon the broad foundations of exact scientific observation; and we shall all welcome such contributions as so able a physicist as Professor Tyndall can make either to our knowledge or to our facilities for testing the foundations of our beliefs. The electric beam, which has in his hands played a large part in many able investigations and demonstrative experiments, was lately brought into play to demonstrate the ubiquity of dust in the atmosphere. To some very charming experiments Mr. Tyndall joined some theories which, if capable of proof, were yet not demonstrated by anything which he said or did. The ubiquity of these airborne particles was perfectly well known, and their illumination by the electric beam, while it has given a more complete demonstration of their presence than was otherwise obtainable, has not added anything to our knowledge of their chemical or biological relations. His experiments, however, have had the valuable effect of demonstrating the uses of cotton-wool as a filter for them, and the advantage of inspiring him with interest in a subject in which we can but be pleased that one of the most brilliant of investigators and expositors of physical science should be interested — the investigation of the origin of zymotic disease.

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"In many respects the address shows a considerable advance over previous discourses by the same lecturer on this subject. The fact that the dirt or dust is in large part inorganic and in large part 'dead,' is now put prominently forward. Prof. Tyndall has, in fact, profited greatly by the lessons of Dr. Gull—we shall not venture to assume that it is by anything which we have had to say by way of comment upon his previous addresses—and does not now assume to tell us anything more about the nature of this dirt than we knew before. He proceeds only to reason upon the subject, deriving his information, however, chiefly from chance communications from various physiologists and medical correspondents. One correspondent tells him that 'blood free from dirt' will take longer to putrefy out of the body, and Von Recklinghausen's experiments are brought in reinforcement of the still more striking results and experiments of Prof. Lister; another informs him that vaccination through a bleb raised by blistering is less likely to produce secondary abscesses than by the ordinary method; and Dr. Budd assures Prof. Tyndall that, 'from the day when he first began to think of these subjects, he has never had a doubt that the specific cause of contagious fevers must be living organisms.' The last is, of course, a very interesting proof of early wisdom, but is not of the nature of a strict demonstration. The circumstance mentioned by Mr. Ellis reminds us that we have, on the other hand, seen it stated in print by one gentleman that he had to abandon vaccination by blistering because it was, in his practice, more productive than any other of suppurative and inflammatory accidents. But all this is really beside the question. The whole course of subcutaneous surgery, the whole range of Prof. Lister's experience, the daily experience of the difference in progress between simple and compound fractures, a thousand facts and observations, and the accepted and proved theories of surgical practice, have long convinced every surgeon that, in proportion as air and that which air bears are excluded from the fluids of open wounds, and from the organic fluids of the body, suppurative and putrefactive processes will be lessened and warded off. So much Prof. Tyndall might, so far as our profession is concerned, have taken for granted; and if he chooses to read, for instance, such papers as those we have published of Adams on subcutaneous osteotomy, he will see how largely this knowledge affects our practice in other directions than those to which he has referred. But, after proving to us what we know, Prof. Tyndall takes a leap, and assumes precisely those conclusions which we are desirous of his aid in testing. All these facts are as much accordant with the doctrines of Liebig and the experiments of Bastian, as with the doctrines of Schwann and the experiments of Pasteur. Granted that air-borne particles are prime agents in initiating putrefactive and fermentative change, is this by a development of pre-existent living germs, a growth of deposited ova, or by a communicated molecular motion of dead organic matter in a state of change? Is it from germs or from fermentative organic particles? We wish we could see that Prof. Tyndall had advanced our knowledge at all concerning this, the central knot of the tangle. It does not help us when he quotes certain known examples of parasitic disease, such as arises from pébrine. Because the itch is the result of the activity of the acarus, it does not follow of course that all skin-diseases are parasitic. Mr. Tyndall declares indeed, that the successful workers and profound thinkers of the medical profession are daily growing more convinced that 'contagious disease generally is of the same parasitic character' as the silk-worm disease. We cannot find on what he bases that very broad statement. Where are the works of the 'most successful workers and profound thinkers' which support that statement? It will be very kind of the lecturer to inform us whom he thus dignifies, and to what growing series of authorities he refers. Certainly not to the researches on cholera of Gull, Baly, or Cunningham and Lewes: these negative the parasitic theory. Salisbury started a parasitic theory for measles, but his observations have been generally discredited, if they were ever accepted. Hallier's observations have certainly not gained in authority by the results of many recent investigations such as those of Burdon Sanderson. We are not aware of a parasitic theory of scarlet fever being held by any one. The theory concerning typhoid fever, which Dr. Budd holds strongly and defends ably on purely logical grounds, is as distinctly controverted by Dr. Murchison.

"Prof. Tyndall, however, lays just stress upon one important aspect of the question, which is precisely that which has long fascinated medical observers, and which is of the deepest importance. To it also, however, he adds nothing; and from it he draws, with admirable and unquestioning boldness, precisely the conclusions as to which we have all been debating whether they be the true and only conclusions. Small-pox and scarlatina are, to use the graphic words of Miss Nightingale, in ordinary medical experience, 'dog and cat,' so that one cannot change into the other any more than Tabby can give birth to Fido. When she says that she has seen with her own eyes one or other spring up in a corner of a room from neglected dirt, Miss Nightingale uses, of course, a purely figurative language, and her evidence must be taken *quantum valeat*. But when Mr. Tyndall declares, on the other hand, that zymotic diseases are all of primal inheritance—long descended primeval germs, never changing, never dying out, and ever passing on by lineal descent—he treads also upon ground less secure than he supposes. That this is the ordinary observed mode of extension of contagious diseases no one will dispute. That they have no other many will dispute. When he declares that, for the similarity or identity of effect of like particles acting on like fluids, we have no physical parallel, he obviously leaves out of view the whole series of phenomena of crystallisation from saturated fluids.

"To sum up: The tendency of modern research is certainly not so favourable as Mr. Tyndall believes and expects it to be to the theory of the parasitic origin of contagious disease. We should rather declare it to be unfavourable to that theory. The theory of the permanency and unrelated individuality of zymotic types of disease is not, as he assumes, an undisputed or unquestioned theory. We have to set against it, first, the theory of the correlation of zymotic diseases, which is growing into importance, and likely to attract more attention now than heretofore; second, the observations of statisticians of the complementary character of epidemics of zymotic diseases, and their apparent interchangeability in periods of decline; the theories of the spontaneous origin of zymotic disease by no contemptible observers, and in diseases as distinctly communicable as typhoid; and the observations, experiments, and reasonings of Pouchet and of Bastian, which have not yet been met, and which cannot be disposed of by a few words of philosophic doubt. We appreciate very highly the value of Prof. Tyndall's assistance in solving these questions. We entirely concur in his opinion that, as a physicist, he has a great power of usefulness in this field of investigation; and, if we refer him to the work of Gull, Baly, Cunningham and Lewes, Farr, and Murchison, it is because we are desirous that he should not be content to win easy triumphs with audiences uninstructed in the questions he discusses, or with the partisans of the theory he has adopted, but that he should enter into the heart of the question and face its real difficulties. It would be infinitely satisfactory if we could all arrive at as simple a sole theory of disease as that which Prof. Tyndall accepts entire, symmetrical, and rotund, from the supporters of the germ-theory; but we fear the solution is not yet in hand. It is satisfactory to have enlisted his sympathies, and we shall all be glad of his solid and sincere assistance."

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